Unedited Transcript

of

# THE BATHYSCAPH CONFERENCE

January 20, 1958
(Morning Session)

National Academy of Sciences
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Committee on Oceanography
Committee on Undersea Warfare
Office of Naval Research

BATHYSCAPH CONFERENCE

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DR. BROWN: Gentlemen, we are here today to discuss a new device which makes it possible for man, for the first time, to literally go to the greatest depths in the oceans, and we are here today to discuss the potentialities of that device, both from the scientific point of view and from the military point of view.

I am reminded a little bit, when thinking of the potentialities of this new tool in the field of science, of the situation which has arisen in the field of physics and the physical sciences in general with the advent of really high-speed automatic computers. Now, high speed automatic computers can obviously be used to solve rather obvious problems, but there are many problems which are not obvious, for which the computers can be used also, and indeed there are problems which would not be amenable at all to solution if the computers did not exist. A physicist friend of mine stated that this made it necessary for the physicist really to reorient his thinking, that so often his past conditioning concerning the types of problems that could be solved and the types of problems that cannot be solved is such that he just automatically dismisses from his mind the supposedly impossible problems, and indeed he dismisses them in his subconscious even without letting them come up to the surface. This in physics

has been a very real problem. By no means has full use yet been made in the use of the computers in the solution of difficult problems in theoretical physics.

In this particular case I believe that the analogy is a good one. On the military side, with a new kind of gadget like this, I believe that we should take a lesson from the past. This lesson I think was well illustrated by a cartoon I saw not very long ago, which showed a caveman talking with another caveman, and he had around him a huge assortment of clubs, all shapes and sizes, some thin, some fat, some with knots on them, some were smooth, and over in the distance there was another caveman shooting a very primitive bow and arrow. The caveman with the clubs looked over at his friend and said, "That's a nice trick, but it doesn't have any military significance."

In 1956 a conference was held, sponsored by the Committee on Undersea Warfare in the Office of Naval Research, a conference on Aspects of Deep Sea Research. That report has just come out, and it was pointed out to me a few moments ago that a resolution was passed unanimously at the conference. I would like to read it to you.

"The careful design and repeated testing of the bathyscaph has clearly demonstrated the technical feasibility of operating manned vehicles safely at great depths in the ocean. The scientific implications of this

capability are far reaching. We, as individuals interested in the scientific exploration of the deep sea, wish to go on record as favoring the immediate initiation of a national program aimed at attaining for the United States undersea vehicles capable of transporting men and their instruments to the great depths of the oceans."

Since that resolution was passed something has been done by the ONR, and this morning we are going to have a series of reports concerning what has been done.

Today, in the first part of our conference, we have a couple of general objectives. This morning we are to consider whether the use of manned vehicles for deep sea research is being properly exploited, and this will include (a) the presentation of scientific results of the recent program of bathyscaph dives and evaluation of their significance, and (b) consideration of the potentialities and limitations of the bathyscaph for further scientific studies of the deep ocean.

We hope to keep the symposium today as informal as possible. We want to have as much discussion as we can possibly have. For that reason I ask the speakers to toe the line from the point of view of time. Following each individual paper there will be an opportunity for discussion, but I hope that you will confine the discussion

at that point to the specific topics under discussion in that particular paper. Following the presentation of the papers there will be what we hope will be about an hour or hour and fifteen minutes for general discussion, so, if you can, reserve your general remarks for that particular period. We were going to use a time clock, but I am a firm believer that that sort of stifles things, so we will dispense with the time clock, and I request the speakers to be careful.

We will first have some introductory remarks by Dr. Iselin, of Woods Hole.

DR. ISELIN: I suppose the reason I was asked to get up and say something is that I probably wear more hats than anybody else in this room.

The Undersea Warfare Committee of the Academy has been interested in this thing and has been thinking about it for quite a few years. We have not thought that the advantages of being able to go to any depth was the overpowering thing. We have thought perhaps it would be nice to get down as deep as the axis of the main sound channel. Then, of course, the Mine Advisory Committee of the Academy has been thinking about getting down and looking at the bottom in very much shallower water.

The other thing I think I might say is that, as an administrator in oceanography, I have probably been the

worst enemy of this thing that it has ever had, and I am just going to publicly admit that probably I think that I was wrong. Before the war we built little ones, and the thing I had against them was the gasoline. This was a messy thing from our little vessels, to have to launch something involving several hundred gallons of gasoline. You put these diesel engines in the ships, or you don't have any gasoline, then it just upsets the whole business if you have any messing around on the deck with gasoline.

Another thing I had against it was, I think, peculiar to our environment at Woods Hole. It's a long way from Woods Hole harbor out to the 100 fathom curve, and the water is too dirty to make use of a manned vehicle near Woods Hole. The currents are too strong. Wintertime is too stormy, and all such things, but it does seem now, looking back on it, very foolish. If we had just put a few dollars a year into building such a thing, we could long ago have accumulated a great deal of experience, but it always seemed each time that the thing came up that there wasn't quite enough science to be gained that we couldn't gain some other way. I feel now that I did make a serious mistake, and that the whole subject of oceanography has grown up enough to the point where not only can you afford to take advantage of the basic idea in this device, but there are certainly clearly, I think,

things that you can't find out any other way.

DR. BROWN: Now we will hear from Gordon Lill, of the Office of Naval Research.

MR. LILL: I wish to welcome the group on behalf of the Office of Naval Research, and to make a few remarks about the interest of the Office of Naval Research in the bathyscaph and bathyscaph-like devices.

About five years ago we could have had the French bathyscaph, the FNRS-3, for $15,000. At that time old Auguste Piccard, the bathyscaph's inventor, was in hock to the French Navy for exactly that amount. He sent us a plea through the Office of Air Research in Brussels, Belgium, to, for God's sake, give the French Navy $15,000 and we could have the bathyscaph. At that time we didn't believe in it. We didn't have the vision to see what possibilities it had, and it just got lost in the shuffle.

About two years after that we had a conference on the bathyscaph, which was initiated at the request of Dr. Tom Killian, who is the Chief Scientist in the Office of Naval Research. He had been taken to Naples by Dr. Robert Dietz to look at the bathyscaph, which is the bathyscaph TRIESTE, which had been made in Italy since they had lost their FNRS-3 to the French. Dr. Killian became quite enthusiastic about it. As a consequence

Dr. Adkins and myself went to Naples and spent a week with Piccard, and invited him over here for the symposium on Aspects of Deep Sea Research, at which time this resolution was passed suggesting that a program in bathyscaphs be organized as a national program. Last summer, or early in the year last year, in 1957, we made a contract with Jaques Piccard to operate the bathyscaph for the Office of Naval Research for the benefit of those scientists who wished to go down in the bathyscaph, and to operate it in the Mediterranean. The contract was a simple one, simply for the operation of the bathyscaph. As you will hear, we had a very successful summer. We got 26 dives whereas we had expected to get only 15. There were certain communication difficulties with the Italians, but I think the Italians really did very well by us. They gave us tugs. We had a tug at our disposal whenever we wanted it, at no charge, and we had various other things supplied through the Italian Navy. For this Admiral Arleigh Burke sent a letter of thanks to the Chief of Naval Operations of the Italian Navy.

I might make a few remarks about the bathyscaph. I think most of you know what it is and how it operates. If somebody will flip on the projector, I will run through this very quickly.

(Slide.) The bathyscaph is a free diving device.

It has no connections of any sort to the surface of the ocean. There are no cables. These cables you see here are fastened to a crane. It is being hoisted up onto the pier.

It works on a very simple principle which has been known for hundreds of years. It is the principle of Archimedes. This big tank (indicating) is filled full of gasoline, which is simply used for buoyancy control. The walls of this tank are about one-fifth of an inch thick. These black lines mark compartments in the gasoline tank. This one is crooked, as you see, to allow for the expansion of the gasoline, since this tank is open to sea water pressures and temperatures. Sea water can come in through holes on each end of this tank. Therefore, this tank does not have to be built to withstand strong pressures.

These devices you see here are additional buoyancy controls. There are tanks inside full of iron shot, which are held in place electromagnetically. If the circuit is broken, the iron shot is released. If anything happens to the power supply, or if the whole power supply on the bathyscaph goes out of commission, this whole tank with the ballast is jettisoned and the bathyscaph comes up to the surface at once.

This tank is roughly 50 feet long and 12 feet in

diameter. The sphere is about 6 feet in inside diameter and about 4 inches thick. I think the steel hull is about 4 inches thick. It's made in the form of two hemispheres which are pressed together. It will protect people inside to depths of 10,000 meters with a safety factor of 1.5, or a safety factor of over 2 or 2.5 at depths of 6000 meters. It has a window in it for viewing, which is about 16 inches across at the outside and about 4 inches across at the inside. The window is set in the bathyscaph hull like this (demonstrating) and is made out of plexiglass. The window is stronger than the hull, and it was designed this way on purpose since it gives the appearance of being a weak point in the sphere.

This is the entrance hatch (indicating). It goes down through a tunnel. You enter the sphere through here, and you have a pressure door which you can close and bolt behind you. There is a window here (indicating), but you can't see out of it once you are inside with the door closed. This tunnel is filled with sea water once it submerges. When it comes to the surface they blow the water out with compressed air.

The bathyscaph operates fairly well in a Sea State 2. It can be towed fairly well in Sea State 2. In Sea State 3 you'd better stay in port. This is rather fragile.

We have high hopes of eventually modifying this craft

so it has greater horizontal mobility, so that the power supply which runs instruments and lights inside this sphere is separated from the power supply which runs this little propeller up here (indicating). At the present time there is only one power supply, delivered by silver-zinc batteries. He has also used lead storage batteries on board. He has had one-ton storage batteries on board and taken it down to a depth of 3000 meters with these. It has a present mobility of about one-quarter of a mile if you want to conserve your power supply for scientific purposes. If you want to use it for moving around, you can probably go about 10 miles with it.

You get a good view through this port. It's a wide-angle view. You are limited in vision by the sea water and by the amount of lighting you can fasten on the outside.

Is there anything else I can say about this, Art?

MR. MAXWELL: I think that covers it pretty well.

MR. LILL: I think I covered the main features of this bathyscaph. The French one is about like this. The sphere on the French one is not as good as this one, since it's made out of cast material and this one is made out of forged material. It will probably never wear out. It's made out of forged fatigueless steel. If you were

to dive this to 10,000 meters the sphere would contract to about 3 millimeters. If it ever did break it would implode. It wouldn't break.

DR. BROWN: Are there any questions about this?

(No response.)

DR. BROWN: If not, thank you very much. We will go on to our first technical paper.

Dr. Russell Lewis will describe some acoustic research that has been done with the bathyscaph.

DR. LEWIS: The Underwater Sound Laboratory's part of the program was in three parts. As many of you know, when the invitation was extended for operating aboard the bathyscaph, in the various laboratories there was very little time to prepare, and before very much could be done several people went to see the bathyscaph in Italy to see what could be fitted to it, in it, and on it. After returning from this trip there was about two months time left to get ready. Therefore, the equipment had to be as simple as it possibly could be.

The phases which the Underwater Sound Laboratory undertook were to make some directional measurements of the background noise present, to provide the bathyscaph with underwater telephone communication, and to attempt to do some correlation work in the deep sound channel. The emphasis was placed on the background noise

measurements. It was not hoped that we could get an area completely free of man-made noise, and we did not. We merely measured the noise that was present at the time of the dives.

If I may have the first slide, Dick, I will illustrate where the hydrophones were placed.

(Slide) Can we get the left end on the screen for a minute?

The line hydrophone on the stern, of course, had a receiving pattern in the horizontal plane. The rectangle above the horizontal line hydrophone, of course, also had a directional pattern normal to its axis. The little square to the left of the hydrophone represents an omnidirectional hydrophone, and ahead of the center structure is a square hydrophone which used pressure gradient techniques to get directionality, and the pattern was roughly a cardioid with the main axis aimed upward.

That's all for that slide, Dick.

The apparatus used to measure the sound intensity was a simple transistor amplifier, a one-octave band filter used for one-octave bands, and a Ballantine meter and suitable switching.

It had been hoped that a reading could be taken about every 15 seconds during the dive. Actually a reading was taken about every half-minute. A certain amount

of time was made by dropping a ballast, which makes noise and prevents the taking of data and underwater telephone communication. The data that I will present here was taken on the deepest dive of my own during the summer, to 9,200 feet, and some very interesting effects were shown here.

(Dr. Lewis exhibited charts to illustrate his presentation.)

(Chart 1) These are depths in thousands of feet. The levels are approximately db above a microbar, and the lowest frequency, the 300 to 600 cycles, is blue. The next octave is red, the next one is yellow, and the next one is white. On this particular dive there are several prominent things. One response at this depth is just a general noise increase. Unfortunately we didn't get data here and here (indicating) at this depth, but this happens to be the axis of the deep sound channel in the Tyrrhenian Sea, which is about at 1000 feet, much shallower than the Atlantic. This particular hump was a small tanker which was disappearing toward the island of Ponza at the time. It didn't disappear as fast as might be indicated here. It just happened that it came in at this level. However, it was gone at the time we reached this depth (indicating).

This is the most interesting, at 4,700 feet, roughly, well below the deep sound channel. There was a very

substantial increase in sound on the vertical hydrophone, which received, of course, in the horizontal plane. It was a quite pronounced increase. This was noted to some extent on the omni-directional hydrophone, but not at all on the horizontal hydrophone, and it was not recognized by the one which received sounds from above but not from below, which says essentially that the virtual source of the sound was at the level of the bathyscaph. This can be illustrated a little better on another type of presentation which I will show you in a moment. This depth was about 4700 or 4800 feet. I will come back to that later.

(Chart 2) Now, by putting this in three dimensions the relationships are a little clearer. This was the sound channel (indicating). This was the small tanker, and the effect of reduced directivity index at low frequencies is apparent from this presentation. Also, there appears to be a pattern in here which wasn't too noticeable on the other type presentation.

I'm sorry. I should have explained my coordinates here. Depth is this way (indicating). This is level (indicating). Frequency is getting higher from dark to light, from low to high frequencies. This, again, is the vertical hydrophone on the stern. This was the horizontal hydrophone (indicating), which had response to various sounds, especially to this small tanker, but did not

recognize, or scarcely recognized, the big increase in sound at 4700 feet, but did show another response here (indicating) which was not indicated by the vertical hydrophone.

(Chart 3) This is the omni-directional hydrophone, and again the 4700 foot level is here (indicating). It is interesting that it apparently showed up on the high frequency end but not on the low frequency end. However, I might say that a very possible cause for this is that there aren't enough points. You can't take too much data taking a reading every half-minute.

The pressure gradient hydrophone, the one which received from above but not below, could be used only at two frequencies because of its band-limiting characteristics, and that is illustrated by this presentation.

(Chart 4) When we were ascending on this same dive it is rather interesting that at about the same depth, 4700 feet or thereabouts, the vertical hydrophone again showed a sudden increase in sound level, but this time it stayed there. The sound level was present during the rest of the ascent up to 2000 feet. What happened beyond there, I don't know. I don't have data. But on the omni-directional hydrophone the same effect is noted but not until the depth is very much less, and on the horizontal hydrophone the same can be said.

Now, this sound we know (indicating chart). It was from a tanker which appeared some two hours after we surfaced. It could be recognized as a ship, and we assumed that the tanker which appeared was the ship that was generating the sound because the sound became continually louder.

(Chart 5) We took similar measurements on other dives. There were four dives available and only two of them could be used for this particular type of measurement. The other one was made south of Capri. The depth was less. We went to 3600 feet, and at about 1500 feet on that dive there was a similar rise in sound level. Also, on the dive at Capri -- I'm sorry I don't have a chart here to show it, but I didn't have time to prepare it -- there was a continual decrease in sound from the surface to the bottom. This is understandable. There it was Sea State 3 and the noise continually decreased as we got away from the surface, and superimposed on that were the various fluctuations.

Dick, will you put the next slide on for a moment.

(Slide 2) This shows for the deep dive the sound channel axis as taken from the temperature measurements made on the thermometer which was in the bathyscaph, and I'm sorry to say that wasn't too accurate, but it was good enough to determine the sound channel depth. This

also agrees with a 900 foot BT.

Next slide.

(Slide 3) The point of interest here is on the left, the series of arrows. These indicate the rotation of the bathyscaph as it descended on the deep dive. As you can see, it behaved in a rather erratic manner. Sometimes it would change direction by 200 degrees in 3 minutes. Other times it would reverse suddenly and go in the opposite direction. The total turning was either one-half or one-and-a-half revolutions -- I don't know which because the data is not continuous.

Is there one more slide? That's all? All right.

The other dives made for the Sound Lab were a dive to test communications, made on the 14th of July, south of Capri, to 3600 feet. It had been intended that that should have been a dive to measure the bathyscaph self-noise. However, the equipment had not arrived, and Piccard was anxious to see how communications worked. He was very pleased, incidentally. We had very satisfactory communication with the surface all summer, using an underwater telephone working at the same frequency and on the same principles as that of the UQC used aboard submarines. However, this was a much smaller telephone, with about 15 watts output.

A dive was made to 300 meters, or 1000 feet, roughly,

to make some measurements at the axis of the sound channel. This dive required that the bathyscaph be suspended at a particular depth, and it is inherently unstable in depth because as the gasoline compresses you lose buoyancy and as it expands you gain buoyancy, and these work in the opposite direction from which you would like. Therefore, something had to be done to gain some positive buoyancy at a particular depth. This was done by putting a 250 pound displacement float on the surface. We connected this to the bathyscaph with 1000 feet of one-quarter inch manila rope. The 250 pound displacement was sufficient so that the bathyscaph ballast could be adjusted and the vessel could be kept at one particular depth. We lost depth control only once, and that was for a very brief time.

The experiment was not a complete success from the acoustical viewpoint, however. We had mounted some motors on the bathyscaph athwart-ship at one end, intending to rotate the vessel so that we could use a correlater to get directional noise information as we rotated at the axis of the channel. However, the correlater depended completely on being able to rotate the bathyscaph. The battery box failed at the last moment. It was hoped we could rotate it using Piccard's own motors. One of his motors failed at the last moment, and it was impossible

to rotate the vessel on one motor. Therefore, the best that could be done was to log the sounds. The directional pattern of the hydrophone is known, and I am still trying to determine which of the many ships in the area we can be certain we heard and which ones we could not be certain of. However, the bathyscaph did operate at one depth and this can be repeated perhaps some time in the future.

I might mention -- the limitations of the bathyscaph were mentioned earlier -- that we actually towed it in a Sea State 5 one day, but we could not recommend it. It suffered some damage. And we made at least two dives in Sea State 3. Again, I wouldn't recommend that. We were soaked to the skin before we got in the bathyscaph.

The four dives that we made, then, were used to make directional background noise measurements, as illustrated by these drawings. Two of them were used for that purpose. One was used for the telephone tests, and one was an attempt to make measurements at sound channel depths.

That concludes my remarks on the subject.

DR. BROWN: Are there any questions?

MR. HARTER: I would like to know at what maximum depth did your 15-watt phone work successfully, and did you have any reflection problems, and were they affected by your angular position? We have an interest for carrier

modulated video.

DR. LEWIS: The phone worked at all depths. There were reflections. As a matter of fact, the telephone could be used as a sort of auxiliary fathometer. It was used to measure ranges between the motor launch and the bathyscaph.

I might mention it had another interesting use. It permitted checking the proper functioning of the ballast tanks. Previous to using the telephone it was necessary for Piccard to look out each window in turn to see that the ballast was flowing smoothly. However, since it could be heard on the telephone this wasn't necessary and resulted in less disruption of the data-taking procedure.

MR. FRESSETO: Was there any explanation for this sound increase at 3700 feet?

DR. LEWIS: For some reason or other the sound was channeled to this depth, which was far beyond the reach of the depth sound channel. I can't say why. I don't have precise temperature measurements, unfortunately, and it wasn't shown on the thermometer that Piccard had. However, the fact that the bathyscaph rotated suddenly at various depths probably indicates currents which may have some effect on the channeling of sounds.

DR. BROWN: Yes? You have a question?

DR. VINE: I think it is quite possible and practical that that could be focusing from a distance -- this source. Without the data here I would only guess about 15 miles or something thereabouts.

DR. LEWIS: The only ship we know of in the area of that small tanker was this large ship which appeared some time after we surfaced, and if that is where the sound was coming from it would have had to travel some 60 miles to have reached that depth. Perhaps it could have been multiple reflections.

DR. VINE: It would only require something like 1000 fathoms of water to permit focusing in that area.

MR. FRASSETO: I thought the nature of the diagram was such that the focusing occurred at the surface and not the depths.

DR. FROSCH: He has a hydrophone on which you look at the horizontal, so you have to look at it and paraphrase it at that depth and go up, and in that sense the focusing is really provided by the hydrophone itself.

DR. LEWIS: There was another interesting phenomena. Perhaps we should expect this, but of course there was an abrupt change in slope with frequency at that depth. All the frequencies appeared to come in at about the same level. I don't really have a good explanation for that. At other depths they seemed to have a more or

less normal slope.

DR. RAFF: You might comment on your ......

DR. LEWIS: On this dive we didn't have it. On the sound channel dive we did, but we did have a fair surface resonance, I would say, at least for anything that could be seen from the top, as from the Corvette as it steamed around -- well, it didn't steam around too much, but we did have a fair range of vision.

DR. BROWN: I think we are going to have to move on now. Thank you very much.

Our next paper will be given by Mr. Morton Lomask, on low frequency acoustic research.

MR. LOMASK: The work of Hudson Laboratories took over in general from the low end of Russ Lewis's frequency range and went down. We confined our measurements to the range of from 5 to 400 cycles. Our purposes were to measure levels, ambient noise levels, as a function of depth and frequency, and also the self-noise of the bathyscaph, to find where hydrophones must be placed to be quiet and where they would be noisy, and also to measure the noise of the ordinary operations of the bathyscaph, such as dropping ballast and releasing gasoline and turning on motors, and so forth.

In the course of our stay we found that we would be able to use a low frequency source from an acoustic

minesweeper, and we thought that this would enable us to get a vertical profile, a vertical energy distribution of a point source at the surface at different distances. We had four dives. First we had an 1100 meter dive off Capri, and this was, so to speak, to get our feet wet. We tried out the equipment there. We attempted to get levels, acoustic levels, versus depth and frequency, and, also, we tried turning on different things in the bathyscaph and we analyzed the noise levels on hydrophones spaced at different parts of the bathyscaph.

The second dive was off Ponza, to 3200 meters, and this was to measure levels and directionality of ambient noise on the way down, which required a slow descent, and on the way up we had the source from the acoustic minesweeper operating at a distance of around 7 miles, and we got a vertical profile of the energy distribution.

The third dive was also off Ponza, to 3000 meters, and it was essentially the same as Dive No. 2 except that now our sound source on the ascent was 3 miles off.

Our last dive was to 1000 meters, off Capri, and that was to measure sound levels as a function of depth, ambient levels, and in this dive we made a determined effort to hold the bathyscaph by its own means at different levels and see how it operated as a vehicle where we, so to speak, pre-set its depth at some intermediate depth not at

the bottom.

I have just a couple of words on the instrumentation and general layout of our scheme. We used 7 barium-titanate crystal hydrophones and we located them in several areas. We had one over here (indicating), mounted rigidly. We had one on the middle of this conning tower, so to speak, and one at this end here, and the spacing was 16 feet and 32 feet, and of course we considered these extreme ones. We had an element in array spaced at 50 feet, and we had an identical system of 3 hydrophones spaced from wires dangling down here (drawing on blackboard), which was more or less an alternative array if things here didn't work, and it also provided us with ideas on hydrophone mounting -- if it was more quiet this way, dangled from wires, or this way, where it was more rigid.

Then we had another hydrophone to give us a vertical array, which was, in most of our dives, suspended from a float or, rather, being pulled up by a float tied down to the bathyscaph and up 50 feet from it, so we would have a vertical array similar to this horizontal array with 16 foot spacing and 50 foot spacings.

We had no electronics external to the bathyscaph. We thought it would be better to have all our electronics inside. In so doing, the thought was that we could build

lower noise electronics if we did not have to compromise with external pre-amplifiers and things like that.

Inside the bathyscaph we had two articles of electronics. One was just simply a dual-channel low-noise pre-amplifier. Then we had our major chassis, which was a bank of filters and integraters and a read-out system, and also a correlater which employed the same read-out system as the other electronics.

All work was done in six different frequency channels. These are nominal figures on the center frequencies of the filter bands: 7½ cycles, 15 cycles, 30, 60, 120, 240. The selectivity of these filters was approximately so that the 3 db point on this curve would be about two-thirds the center of frequency. In other words, the Q was around three-halves, and we endeavored to make all these filters with this identical Q. When we analyzed a sample of noise we charged the integrating condensers for 60 seconds, for one minute, and we charged them simultaneously. In other words, we had an integrating system for each channel, so the whole spectrum analysis was taken on the same sample of noise integrated over one minute. I mean, we did not take a reading of 240 cycles at a level reading and at a time later take a sample. I think this is significant because, as the results indicate, there are sharp fluctuations in the course of a minute or so.

The correlater was a sine correlater. We had the outputs of two hydrophones go through these matched filters and then the signals were clipped and the phase of the axis crossings were compared and measured, and this was our correlation scheme. So much for the instrumentation of it.

I want to say at the outset that the bases of what follows, the results I am going to mention, are four dives. We had gaps in these dives and, therefore, the information is not exhaustive or encyclopedic, but certain things do stand out and are worth mentioning, but it has to be qualified by mentioning the fact that there are just four tests involved.

First, we will consider the spectrum of ambient noise at various depths (drawing on blackboard) where this axis is db below a dyne per cycle bandwidth, a dyne per square centimeter per cycle bandwidth, and this one here is frequency (indicating). The typical spectrum that appears is something like this at a deeper depth. The highest energy per cycle bandwidth is in the low end and it decays with the slope of something like 6 db per octave, and this brings us down to around 25 cycles. At this point here -- well, this is very gradual. I shouldn't say "at this point," but we seem to lose some of this slope. It levels out, but it still

decreases, and this goes on up until a little over 100 cycles. Then there is a suggestion of an increased slope again. Sometimes this appears to a greater extent and sometimes it's not so obvious. For reasonably quiet conditions -- in other words, low sea state number or for operation right at the bottom -- the reference line here is something like minus 30 db below a dyne per square centimeter.

Now, we also were able to get curves of ambient levels versus depth, where this would be the surface and here would be the bottom (drawing on blackboard), and here we would have units of energy. In other words, this would be a simple filter band, maybe a $7\frac{1}{2}$ cycle filter band, or maybe it's the 30 cycle, and so forth. The curves that I have gotten had more fluctuations than in Russ's curves that we just saw before. The structure in my case was quite a bit more irregular. I will say more about that in a moment.

We observed that for different frequency bands -- I mean, to draw a straight line through here (drawing on blackboard) you would get, in general, a different slope of this line. In general, of course, it was noisiest near the surface and decreased monotonically if you drew a line through the average of these fluctuations to the bottom, and a straight line actually was

not a very bad representation. For low frequencies, below 30 cycles, this line was more like this (indicating) and the pattern was very definite. For the lowest of frequencies, for $7\frac{1}{2}$ cycles, it was almost vertical. There was no particular preference for the low frequencies to any depth. For higher frequencies, like 60, or 120, or 240 cycles, we observed that those frequencies favored the surface and decreased monotonically to the bottom. We were fortunate to have curves of this taken for different sea states, and of course the greater the sea state number the greater the rate of increase of noise as you go to the surface. Sea State 2, for example, would have a slope, or a rate of increase, around twice as great as Sea State 0 or $1\frac{1}{2}$.

Now, these fluctuations in my data were of the order of 50 percent of the mean value. In other words, if you consider this point here, you would get fluctuations around one-half the leve, 50 percent of the level, and these fluctuations are rapid. In other words, if you make one spectrum analysis separated from another one just two minutes away, you might be 25 percent off.

On our directionality measurements the results are difficult to analyze, and we are doing this now. I am not going to say anything on it. We do have the data on it, though, but it does not suggest anything obvious.

For our vertical profiles -- I will erase this (erasing drawings from blackboard) -- we had a sound source, which was this noise maker from the minesweeper. It was a hammer-box with a fundamental of 60 cycles, but it also was rich in harmonics which just happened to coincide with our filter bands. This was very fortunate. We would have this source, and off at a distance the bathyscaph would -- actually we did all these measurements on the ascent -- the bathyscaph would go up and, in so doing, it would take a vertical profile of the received source signal. When the source was 7½ miles away -- actually this figure 7½ is not completely accurate -- it might have been 6 miles away, because the bathyscaph did not go vertical. It went off to the side quite a bit. But let's call it 7½ miles. When it was 7½ miles off, we observed a very obvious concentration of the sound energy in the sound channel, which, on the data that I took, was indicated at around 400 meters, and it was an asymmetric sound channel and fell off very sharply on the top side.

Maybe I can quickly sketch in something of what it looked like (drawing on blackboard). This was around 300 meters, and this around 400 meters (indicating). There seemed to be little energy above 300 meters, and a very, very clear sound channel here (indicating). Also, since we did have the advantage of the source not being

multi-chromatic, but having 60, 120, and 240 cycles, we were able to observe if the concentration, let's say, in the sound channel was frequency affected, and we got indications that they were. For instance, 120 cycles seemed to be more definitely concentrated in the sound channel than 60 cycles; 120 cycles here was predominant over 120 cycles.

On another dive we had the source spaced at 3 miles away, and we did the same measurements. Here we noticed no concentration at all in the sound channel as we showed here. In other words, the concentration in the sound channel apparently took place somewhere between 3 miles and $7\frac{1}{2}$ miles. Again we noticed that the energy near the surface was low, and again we noticed a frequency dependence of this energy distribution. This distribution was rather complicated. 60 cycles in the 3 mile case seemed to stand out at lower depths, and the higher frequencies stood out when you were in shallower water. In our distribution of ambient noise in general we did not notice a concentration of ambient noise in the sound channel. In Russ Lewis's work we did notice a suggestion of it, I believe.

In this low frequency analysis I did not see any definite suggestion of a sound channel as indicated from

ambient noise levels. If anything, I noticed a tendency in all four of these dives to have some kind of a channel around 900 meters. I was just hoping that the channel you discovered was around there, but it wasn't. In my data there is a definite indication of a noisy layer around 900 meters. As far as what this could be due to, it doesn't seem to be merely a function of time. At that moment we passed 900 meters a lot of noise came by and just sort of disappeared. In general, whenever we held the bathyscaph at a fixed depth we got pretty regular readings time-wise, so the suggestion here is that either there is actually a noisy layer there in low frequency noise or else it so happens that the bathyscaph just makes noise when it goes to that depth. I don't know.

On general things about the bathyscaph, first of all it doesn't seem to go up and down perfectly straight. It might go down at an angle and come up at an angle and end up a mile away from where you thought it was. This might be attributed to the fact that in general Jacques Piccard, I think, let out ballast from one ballast tank and not from another. I don't know what his reasons were, but this might account for some of it anyway. The usual operations of the bathyscaph, such as dropping ballast or releasing gas, are noisy and, in general,

prohibit making any kind of ambient noise measurements, certainly.

I mentioned that in our last dive we made a definite effort to hold the bathyscaph constant at some intermediate depths. I believe the numbers were 250 meters and 350 meters, and we were successful in doing this without any external means. It was not easy to do. Piccard had to continually release ballast and gasoline to adjust the level, but once it was adjusted it stayed pretty nicely put to within 10 meters for a period of 20 minutes without doing anything, enabling us to make some good measurements.

That is all I have to say at the moment.

DR. BROWN: Are there any questions?

DR. LEWIS: I might mention I do have a case of the noise rise at 900 meters. It's one of the smaller ones.

MR. LOMASK: I have observed this on all four of my dives. In one of my dives this was an indication of a sound channel where it was computed temperature-wise, but on other dives it could not show. The only thing that seemed consistent was a sound channel or noise channel at 900 meters.

MR. MAXWELL: I'd like to ask you a question on your dive on which you had the sound source, the noise maker. You said that the sound that you picked up in the second

channel was frequency dependent. Am I correct in thinking that this might be an indication of dispersion in velocity of sound in sea water, or is this just due to the attenuation at those frequencies?

MR. LOMASK: I doubt if it's attenuation, because attenuation would not play any part in these ranges. At most we were 7½ miles away.

MR. MAXWELL: I will ask Bob Frosch if this is an indication of dispersion. Is it not?

DR. FROSCH: Not the bulk velocity of sound being dependent on the frequency.

DR. VINE: Is this frequency getting low enough so that it might have, maybe, a difference in the degree of diffraction?

MR. LOMASK: There was 120 cycles just seemed to love the sound channel. 60 cycles was more spread out.

DR. VINE: Would you like to clarify a little bit your statement on the difference in the ambient noise between the surface and the bottom in the different sea states?

MR. LOMASK: If you take the output of a filter band, the rectified, integrated output -- these were all based on one minute integrations -- and plotted your points like so (indicating), you got a real jagged looking curve to the surface, where you could draw a straight line through

with a little imagination, and if we consider one frequency -- let's say 60 cycles -- and if we consider Sea State 0, we would get such a straight line. If we consider Sea State 2, we would get such a straight line with, of course, fluctuations around that like this (drawing on blackboard).

MR. SHEVILL: Would you imply that the bottom would remain the same?

MR. LOMASK: The bottoms were pretty close.

DR. FROSCH: Is this on the db plot?

MR. LOMASK: No, this is linear.

LCDR. KALINA: Was a sound velocity meter used, or did you compute the velocity profile?

MR. LOMASK: I didn't compute velocities at all.

LCDR. KALINA: In determining where the sound channel is?

MR. LOMASK: We did take BTs.

LCDR. KALINA: But you didn't measure the sound velocity profile?

MR. LOMASK: No. We didn't measure any velocities. The sound channel that I have observed from this case -- in other words, where the 120 cycles and 60 cycles seemed to be concentrated when the source was $7\frac{1}{2}$ miles away -- we got a very sharp curve over here, which centered at 400 meters, so that is what I called the acoustic sound

channel. I didn't make any velocity measurements.

CDR. BISHOP: When you covered the bathyscaph you took ambient noise measurements in the same manner, didn't you?

MR. LOMASK: Whenever we did this. In other words, this was like our measurements on the bottom. We did not get these 50 percent fluctuations. We got maybe 20 percent, maybe 10, so this leads me to believe that the fluctuations are not time-dependent. Either they are depth-dependent or it's some self-noise of the bathyscaph.

CDR. BISHOP: On these plots that you were making here, where you show a difference in level from the surface to the bottom, did you find any difference in that slope as a function of the ascent or descent, or were they pretty much in agreement?

MR. LOMASK: We only took these on descents. Our differences in general -- I don't know if we made the wisest use of our ascent and descent, but we took a very slow descent, concentrating on ambient noise levels, and since this used up so much time we figured that we would only have left a pretty fast ascent, which was sufficient for measuring this sort of propagation deal over here with the source, so we went down to measure ambient noise and came up to get the profile in the same

dive.

CDR. BISHOP: The reason I ask is that on these plots of Lewis's there is a very definite increase in level on it.

MR. LOMASK: I would like also to mention one point I believe I forgot, and that is hydrophone placement. We found that the hydrophone here, I guess (indicating), both on the ascent and descent, was caught up in the turbulence of the bathyscaph and was quite a bit noisier than this one and this one (indicating on blackboard drawing). It depends on whether the thing is going up or down which way you get more turbulence. You are either in the wake or the leading edge.

DR. BROWN: They are waiting to serve us some coffee out there. We are somewhat behind schedule, so please don't take more than ten minutes.

(Thereupon, at 10:40 o'clock, A. M., a short recess was taken.)

AFTER RECESS - 10:60 A. M.

DR. BROWN: We will now move on to some of the observations that were made on the bathyscaph of a biological and geological nature, and the first paper will be given by Andreas Rechnitzer. He will discuss the biological and oceanographic observations.

DR. RECHNITZER: My primary mission at the Navy

Electronics Laboratory is to investigate the biological factors that interfere with the propagation of sound. Since we had an integrated program most of the acoustic part has already been given. Those people obliged me by taking that particular facet.

One of the principal contributions of the bathyscaph is to permit visual observations, and these we made ample use of. Visibility, as has been explained before, is quite good outside of it, through the plexiglass window. Although it is 5 inches thick it is truncate in shape and permits a 90 degree or more norizontal visibility.

The bathyscaph is also equipped with sets of mercury vapor lamps outside. These can be illuminated at will, and in the beginning were only used intermittently because of their expected short life, but this was corrected later on and they were permitted to stay on for periods up to one hour. Leaving the surface, ambient light, of course, is great enough to provide good illumination in the surface areas, and the Mediterranean is recognized for its crystal clarity, but as soon as you submerge below the surface you begin to see the small particles that exist even in the clearest water. Descending slowly in the bathyscaph the light, of course, diminishes and we reached what might be termed the "twilight zone" at about 350 meters. In my

material what I can see with the assistance of sunlight alone is not too great relative to what you can see once the artificial lights were turned on. Usually we reserved them until we reached about 500 meters. Even to a depth of 600 meters it was still possible to perceive the silhouette of the bathyscaph. Once we reached 500 meters we turned on the outside lights. At that time the abundance of what we called "snow" appeared immediately. The single effect of having a sharp light go on revealed these minute particules. These have been observed by Beebe and Barton and they applied the term "snow." In this case we have "snow" which is falling upward instead of down. They appeared inanimate, and bioluminescence doesn't become obvious until you reach the darker zone. At about 500 meters, in this twilight zone, bioluminescence picked up markedly and as we descended down to the maximum depth -- I achieved 3500 feet -- bioluminescence decreased somewhat. It was obvious all the way to the sea floor. This particular year evidently it is not as spectacular as last, wherein Piccard tells me there was a spectacular display of bioluminescence all the way to the sea floor, caused by salps or tunicates.

I made three dives, to 3500 meters for two, and one to 470 meters, and in all cases we saw a continuous distribution of life, from the surface to the bottom.

Most of the material was small and, as I said, inanimate. There was some indication of movement once we reached the sea floor and settled down into a more or less fixed position. Here then we could see the smaller particles moving around slightly, and, of course, the larger animals, such as fishes, could be viewed quite clearly.

We of course looked for a scattering layer. We had no idea of its presence before we left the surface, as no echo-sounding equipment was available. Had we suspicioned that the Italian Navy was so deficient in echo-sounders we would, of course, have brought one along. There was no way of obtaining one in adequate time for this program.

The most interesting observations were made on the sea floor itself. The Mediterranean is generally recognized as being rather deficient in animal life, but on almost every dive fishes were seen on the sea floor, even to depths of 9000 feet. Although there weren't great numbers of the common genus, cyclothone, a small fish one or two inches in length, they were seen more often than any other. The hatchet fishes were not seen at all, which was a surprise to us because they had been reported by the French, but we did see on our first dive, when we turned on the light, a nice big deep sea cod swimming lazily around. He moved very slowly, without any

particular concern for the introduction of this very bright light, and finally moved out of sight.

On another occasion we saw the largest fish of our program, and this was a 6-foot long, or 2-meter conger eel. It didn't remain in the area very long, but we were fortunate to get some pictures of it.

Outside we had an Edgerton camera, which could be controlled, and we took pictures every five seconds. This was to provide us with 35 millimeter still photographs. One of the chaotic things that happened during the summer was that, after hand-carrying this valuable material back to the Laboratory, it was viewed before it had been developed, and all was lost.

The most abundant animals on the sea floor were the isopods, of one particular form and about a half an inch long. These were positively phototropic, attracted to the lamps, and at times accumulated by the hundreds. We had gotten some indication of them before by the photographs particularly by Edgerton, whereas these appeared in the photographs as little white spots, as they did in our own, but they are there in great abundance, and the fact that they are there, along with these other fishes, indicates there is an abundance of food.

Holes were common everywhere that landings were made on the sea floor. Most of them were unoccupied. At least

this is the way they appeared, although they had all the appearance of being rather fresh. Most of them were about half an inch across and 2 centimeters deep. They also occurred in great clumps or patches, but there was no consistency in the number of holes or any asymmetry, so we can only surmise what animals can live in them.

We also noticed nemerteen worms extending out of their holes and feeding on the sea floor. Active evidence of animal life on the sea floor was almost non-existent. The sea floor was mud and showed evidence of animals having been there by holes and fine tracks and movement of the bottom sediment, which brought one particular thing to mind, and that is the emphasis that has been given in recent years to interpretation of deep sea cores and what chronological history can be derived from these. It was very obvious from the location we were working in that these surface layers were really worked over by bottom-living organisms. The material sediments as far down as a foot or more are brought to the surface and then redistributed by the existing current.

It was quite a pleasure to find that on the sea floor, and in this fixed position that the bathyscaph takes, that a current from about eight-tenths to 1 centimeter per second was normally present. This is south of Capri and the direction of the current was almost always 270

degrees true. West of Capri the current was about the same rate but about 180 degrees true, so sediment south and west of Capri probably receives a great contribution from the Bay of Naples and the Gulf of Salerno. We could see clouds of material passing through the illuminated areas that were outside of our sphere and where we would stir up mud by dropping ballast and the landing of the bathyscaph itself.

Organisms in the water would seem to be passively carried along. The shrimp, medusae, worms, and even the fishes, seemed to rest lazily in the water and were carried along with it. More often than not we detected their presence in the water by their shadows on the sea floor. Observing them directly can be something of a chore. There were some colored shrimp seen up off the sea floor a few hundred meters, but for the most part they were quite scarce.

Going back to the current, the discontinuities encountered by the acoustic people who were in it -- the bathyscaph would rotate some tenths of degrees, even as many as 50 per minute -- would suggest there is some external force acting on the bathyscaph. These we had hoped to investigate, using a very sensitive electronic thermometer which was measured to a hundredths of a degree, Centigrade. Unfortunately, the sea water penetrated into

the sensitive element and rendered this inactive. Following other dives they were to pick me up and load on my equipment and go back out, but the Italians decided the weather was becoming temperamental and they couldn't risk getting caught so far from shore in the bathyscaph. These are something we would like to go back and look at. Also, a fine photograph brought back by Piccard from one of these dives indicates that there are additional forces active, and I have that here to show you, in that there are some extremely large ripple marks, if we can call them that. They are 5 meters, 15 feet, from crest to crest, and also fairly high.

We also installed a plankton sampler, but due to the rush of things it decided not to work at the last moment. This would provide us with something at whatever depth we wish by remoted control within the bathyscaph.

May I have the slides?

(Slide 1) These photographs were taken from within the bathyscaph, which gave us, then, some record, since all of the Edgerton photographs were lost. The clarity of the photographs taken through the plexiglass window was amazingly good. These are the ripple marks seen at 8000 meters. The bathyscaph was sitting atop one of the crests. Then there is a trough over to a crest, then over to a trough and up to another crest.

MR. LILL: What depth?

DR. RECHNITZER: 3000 meters. Then, what is probably not too clear, there is a very large mound here that has undoubtedly been brought up by animals and some pieces of detritus that has come down from the surface. The sea floor for the most part was very clean, and we could not determine what the animals might be feeding on, particularly these abundant isopods. In it there were fragments of the kind of grass which grows adjacent to the islands.

(Slide 2.) These are isopods. This is at 3500 feet. We were sitting on the sea floor. We made a rise to get off the guide rope and settled back down, and we came down within one foot of where we had taken off. This mud was extruded from underneath the sphere. We landed in the same spot, just offset enough so we could see it. I will defer talking about the mud and let Dr. Dietz tell you about that.

(Slide 3) This is a pile of ballast that was dumped earlier. Here, not clearly shown, are a number of light spots, which are the isopods. These have been plaguing us in underwater photographs for some time. Edgerton has photographs from 2700 feet down and they also show crustaceans of various types, some of which include these.

(Slide 4) Here are samples of the holes. The life on the sea floor was easily studied from within the

bathyscaph and is one of the primary things that the bathyscaph can be used for.

There was one other objective to the program, biologically speaking, which was to determine the size, distribution and response of these animals to various stimulae, such as touch, being touched by the bathyscaph. We saw there was some stimulation and response by bioluminescing organisms, but not a great shower as many of you have seen on the surface going along on a dark night. The light had virtually no effect, except on one fish, and he disappeared in great panic.

We used the underwater telephone frequently, and sound generated by this 12 KC instrument didn't seem to have any effect.

DR. BROWN: Are there any questions?

DR. GALLER: Dr. Rechnitzer, from your experience on the bathyscaph, would you feel that this offers itself as a nice way, or an efficient way, of studying the geometry of these particular aggregations, from a three-dimensional point of view, something we have not been able to do very well from a plankton sample?

DR. RECHNITZER: We have an underwater camera, and once this has reached a better state of development it would be wonderful to attach this to the outside of the bathyscaph. The TRIESTE offers the opportunity to select

the area where you take your photographs and, once observing the suspended material with the outside lights and being able to select with the plankton sampler and to take these photographs, I would say it would.

DR. GALLER: Did you correlate your visual observations with acoustic observations horizontally?

DR. RECHNITZER: No, because we didn't get into a detailed enough program to know exactly what the intensities of our sound sources were. Later on we hope to be able to use the length of the bathyscaph to make measurements of attenuation, velocity changes at least, across that distance throughout the entire water column.

DR. BROWN: Are there any other questions?

(No response.)

DR. BROWN: If not, we will move on to our next paper, on the geological observations and other observations made by the TRIESTE, by Robert Dietz, of ONR.

DR. DIETZ: If I may, I will take a little liberty with that title and say some other things as well, but I might say first I am with the Office of Naval Research, London, and our mission over there is, of course, to be a listening post for developments of interest to the Navy.

This bathyscaph, when I first came over there, was something I considered to be an important breakthrough, so I have prepared a couple of technical memoranda

regarding it, and also a translation of some French results on their dives. I might pass this around, and if anyone is particularly interested in receiving a copy I will see that they get it if they will just put their names on this particular memoranda.

(Dr. Dietz presented the memoranda for circulation among the attendees to the symposium.)

DR. DIETZ: The other memoranda I am prepared to pass is more or less of historical interest. First, in regard to the French results, I might just point out in passing that the French, as of last June, had made 52 dives. Piccard now, with our 26 dives this summer, has made 46, and the French have made some more in the past summer, so perhaps they have about 56 dives. The French are now building a new super-bathyscaph, designed to go to 10,000 meters, a three-man affair, very much along the lines of the older one, with bigger engines, bigger electric power, twice the amount of gasoline as the FNRS-3 has, and also the windows will be made so that they come to a very small aperture inside, so it would be necessary to have an optical arrangement to view through this window. This is necessary because of the high pressures of the 10,000 meters to which they expect to dive, and they presumably plan to dive in the deep trenches because this factor of 10,000 meters is really met with there. The ocean is

usually only $2\frac{1}{2}$ miles deep.

I have copies of my particular dive, and I think rather than go through it I can best pass this out and you can read it at your leisure. These are some of my impressions during this one dive I was able to take.

(Dr. Dietz presented more papers to the attendees.)

DR. DIETZ: Also, another publication of interest is one prepared by Piccard and myself, which has just come out on deep sea research, which is entitled "Oceanographic Observations by the Bathyscaph TRIESTE, 1953 to 1956." This only takes us up to the time preceding the ONR sponsorship last summer, and the purpose of this was to bring together some results that Piccard had obtained, that had never been published, so they would become a matter of proper scientific record. We put this out and if you are interested in this I can send copies to anyone. I believe there is a copy circulating of this and if you will put your names on that I can send that to you.

I have one more item to hand out. Here are some photographs Piccard has taken with black and white through the window of the bathyscaph. I will pass these around and you may look at those as well. It will give you some impression of the bottom. I'd like to have those back at the end.

I have been asked to comment on the other observations. I believe this means the foreign observations. I'd like to point out first that it was originally written into the contract with Piccard that European scientists participate in this program. This was done in view of the fact that ONR was putting only $40,000 into this project, which was just enough to cover the summer's operations and made no allowance for the capital investment in the craft and amortization, and so forth, but, more important, because although the TRIESTE is owned by the Piccards, the Italians, and the Swiss -- people don't generally realize this -- they each consider it as their own, and it would have been very undiplomatic, for one thing, not to include them in this, and, also, the program depended on the collaboration of the Italian Navy for the use of the tug, the Navalmeccanica and the company AGIP for the loan of the gasoline, amounting to 28,000 gallons, so it was understood originally that Europeans could participate in half the dives. In actuality 9 dives were made by Europeans, the rest being made by Americans, of the 26. This included 4 by Italians, 3 by a Swedish oceanographer, and 2 by Swiss nationals. The Italians were, in three instances, Naval Officers who were interested more or less in the naval applications of this craft, and one was a Professor Diceglie of Bari, who took down a gravimeter and during

his dive, I understand directly from Dr. Rechnitzer, he made 7 stops going to the bottom, a depth of 820 meters, in order to try to use this gravimeter by hovering. I understand this was done successfully. Also, he took observations on the bottom, which shoed we could use the gravimeter in the bathyscaph. This came somewhat as a surprise to me, because it seemed to me that although the bathyscaph is very stable and below any "seat of the pants" detectability of the accelerations, I thought it would be too much motion to attempt to use something like a gravimeter.

Professor Jerlov of Gothenburg took three dives, none to the bottom, all to moderate depths up to 600 meters, to measure light penetration, and this he has done very successfully and wrote me a letter, but I don't think the details are important here. He found, in short, that it could be uniquely used for certain purposes, especially for measuring the scattering of particles in the sea.

Now the two Swiss nationals were a Michael Kobr and A. Botteron, I believe his name was, both from the University of Lansanne, one a botanist and one a geologist, who made dives to from shallow to moderate depths. So much for that.

To pass on to some of the geological observations,

just briefly I would like to point out that the bathyscaph has three general uses. One is for getting away from long conducter cables and putting rather elaborate equipment inside, and this is mainly of interest to the acoustic people. It could be used either for visual observations or, secondly, for taking samples by some sort of various devices with respect to what you see, which is quite a different sort of category, I think, in usage. This taking samples with respect to what you see requires a more sophisticated instrumentation than what we have. It was only partially achieved. Rechnitzer had a plankton sampler he could operate with respect to what he saw and some sampling was done on the bottom.

The pure visual observations are more of interest to the marine geologist and biologist than they are to the physical oceanographer. I am not very amenable to visual observation -- correction -- the zooplankton are not very amenable.

We had no echo-sounder aboard the Italian motor ship and, also, there was no previous knowledge on the general marine geology or even the biology of the area, so it was difficult to really plan a useful dive. In fact, on my dive I had hoped to go down at the base of a steep slope, but we landed out in a flat plain and on soft mud bottom. On my dive I considered the water to be stagnant, but on

later dives currents up to eight-tenths of one centimeter were observed by other people.

As you heard in one case, there are large ripples on the bottom, giving visual indications of some currents operating. This is exceedingly interesting, since the Mediterranean is essentially a non-tidal area, and it is thought that the currents in the Atlantic and elsewhere must be principally tidal, but here we have currents which are probably not tidal and require some other explanation.

I might point out, regarding physical observations as well, the French have reported a crystal-clear layer of water right close to the bottom, within a very few meters of the bottom, and I think the French have in fact done this. Their observations have been carefully made, and we did not find this in our dives at all, to my knowledge, although this observation must be very carefully made, because once you see the bottom you are so intent on the bottom you tend to forget the water itself and you don't look at the scattering particles in it. Rechnitzer and I tried not to be overcome by our interest in the bottom and we did not discover any crystal-clear layer within some area of the bottom. The bottom on my dive was soft mud, with a good many holes. On other occasions rather firm bottom was found. You saw the one

where the pile of ballast was resting on the bottom.

A rather interesting aspect of this bottom on my dive was the remarkable fluidity of the bottom mud. It isn't plastic and coherent like ordinary terrestrial soils, and I am sure this is because of the different sort of weathering you have on the ocean bottom. When we would drop a bit of ballast we would get a doughnut column of mud. This to me was quite convincing regarding the possibility of turbidity currents, which are now widely invoked to explain marine sedimentation peculiar in cable breaks, and so on, but once you see these clouds set up you become quite convinced that such turbidity currents can be quite easily set up. If there was a slope it is easy to visualize how this might begin to move down and pick up more materials and develop a real turbidity current.

The TRIESTE has floodlights, two spots forward and two spots aft, coming down from above. The water is always very clear. It was full of scattering particles but it was very clear, and we could see as much as 60 feet. The limitation was not imposed by the clarity but by the strength of the lights. With stronger lights we could have done better, I am sure. One thing about these lights, they are good for general viewing but not for viewing the plankton in the sea.

Several years ago I had an opportunity to go down in a Japanese diving bell, the KUROSHIO, off Japan, and on that occasion we had a beam of light right below us going out like the headlights of a car. We could focus and see, "Well, there's a copepod." I couldn't do this on the TRIESTE. We couldn't identify what they were and, due to the fact we were moving, we couldn't tell if they were moving or not.

One thing about the biology Andy didn't bring out was that we confirmed the French observation that you have kind of a maximum of "snow" of these particles at about 500 or 600 meters. Then it thins out again. In general the French reported that the water near the surface becomes clearer, but I, and I think Andy, found this may not be valid, because you can't see these scattering particles when there is a lot of ambient light around. If you would dive at night in the TRIESTE you could probably make a true assessment of the amount of scattering particles all the way down. The amount of stuff you see in a comparatively sterile sea like the Mediterranean is indeed surprising.

It is generally known, of course, that you have an upper layer in the ocean, known as the euphotic layer, where plants can grow, and below that you have quite a different transition to the zone where only animal life

can exist. I don't believe it is generally realized that there is a very important second transition in the ocean, and this is down at the level of light penetration, and this perhaps is -- well, to the human eye it was 500 meters in my case, and for Rechnitzer 700 meters, and to deep sea fish it might be 700 or 800 meters -- but below a certain level there is a sudden transition in the ocean, and all the animals above that are controlled by -- their rhythms are controlled by light. They have a sense of day and night and they migrate, and the whole problem of the scattering layer comes in there, but below this layer you come to a zone where there is no light and, therefore, no time. All time is measured really in terms of light, of astronomical quotation and revolution, and, also, there are no seasons and the animals have trouble, I suppose, knowing when to spawn, and that sort of thing, but this is quite apparent to me, that we entered the zone down deep where there is a remarkable transition in the whole life down there, and this is something, I am sure, that the future of the bathyscaph will open up -- this field of observation in this transition zone from the upper part to the lower part of the ocean.

Now, the geological observations are not particularly impressive, and I hadn't expected them to be, because the TRIESTE is not a research ship or tool in being. It is

something that has to be developed further. This is something we realized. We only wanted to demonstrate that it could be used and that it had great possibilities.

The reason that we couldn't do very much in the way of marine geology is that we had no echo-sounder to spot ourselves and no previous knowledge of the area, like we would off California, for example, where we know it very well from other data.

Secondly, although in theory the TRIESTE can move along and has propellers, and so forth, in actual fact, for the purposes of this operation we couldn't use this horizontal propulsion because we were involved in making a series of dives and it took a week to charge the batteries, so we had to conserve all the electric power. In the TRIESTE these are carried internally in the cabin and are sealer cells. In the case of the FNRS-3 the French have large lead batteries external, which can be jettisoned, and they have a good deal more power to work with, so their horizontal ability is much greater, and with their new super-bathyscaph they will even increase this non-electric power. So the value, of course, of these observations on the deep sea floor is in being able to make a horizontal traversal and being able to see the sediment and eumorphology, and this involves a whole spectrum of features.

One of my overall impressions of this dive -- and I

think of everyone -- was the friendliness of the deep sea. There is, of course, this hydrostatic pressure to deal with, but once you see how the bathyscaph is constructed you see that the problem has been solved.

Our difficulties in operation were not involved with those of pressure sealing. Our difficulties were involved with Sea States 2, 3 and 4 when operating on the surface. It is quite apparent that the limiting difficulties were the inability of the craft to cope with the rough surface, and if we only had a bathyscaph which could leave port and go down deep right away, we'd have it made. If we had one which could be a bit smaller and carried piggy-back on a mother ship, so you could take it out and put it down on a nice day -- I have a few comments here regarding -- well, I suppose these should be deferred to this afternoon -- regarding the modifications of the bathyscaph. I will defer these comments, then, and close now.

MR. LILL: Is there any indication that this "snow" is that of a chemical precipitation?

DR. RECHNITZER: I am personally convinced it is biological. It's not a sediment. It's flocculated organic material, because it's flocculated like snowflakes.

MR. LILL: Is there any indication that at 300 meters, where this seems to be concentrated, it has any effect in

the acoustic work? He said it seems to be the heaviest there.

DR. DIETZ: 500 to 600 meters.

DR. GALLER: May I speak to that for a moment, please? This is purely an intuitive guess on my part, but I strongly suspect that we will find that there is acoustic relationship between this so-called "snow" and your deep scattering layer. I come back to Dr. Dietz' comments earlier that he felt these particles were not large enough to play an important role in the deep scattering layer. I hope my interpretation was correct on your comments. This may well be true. On the other hand, it overlooks the fact that many times these organisms, although individually too small to represent significant acoustic targets, really are concentrated in clusters or aggregates which collectively may represent an acoustic target of some significance.

We carried out some preliminary measurements in a completely different area, and we find that if you were to calculate the real size from the acoustic size of some of these particles you would be completely misled. They would appear to be much larger than they actually are, but the fact is they occur in aggregates and, therefore, they do represent acoustic targets of some significance.

DR. DIETZ: Regarding this scattering layer, the definition is generally defined as something which migrates, and I don't think these would migrate. I presume -- and I am quite sure -- there is a biological population at this level, too, which is quite thick, of small fish, which generally you get on the echo-sounders, but I am convinced that what you say is true. Infinite particles will scatter sound, although the sound people sometimes tell me No, because we have made experiments echo-sounding on the mud layer. You get a beautiful echo from the mud clouds.

DR. FROSCH: It's more likely to be a function of the density of the individual particles. From the description one would guess these particles were pretty much the same. In the mud layer the individual particles differ quite markedly.

DR. BROWN: Are there any other questions? If not, we will now start what we hope will be a group discussion. The discussion will be led by Dr. Allen Vine, of Woods Hole, and by Arthur Maxwell of ONR.

If you two gentlemen will come up here, it would be better.

Within our Committee on Oceanography, which has just been established in the National Academy of Sciences and of which I happen to be Chairman, we are establishing now

a panel for the purpose of making recommendations concerning machines like the bathyscaph, and Dr. Vine is going to be Chairman of that particular panel.

I would propose that you just fire questions directly at them or, if you want to give a comment yourself, feel free to do so. Please state your name, and please speak loudly so we can hear you.

MR. MAXWELL: Before we start, may I just make a comment or two, to perhaps evoke a little discussion?

In the course of evaluating our scientific results and possibly going through some of the difficulties involved in obtaining these, perhaps we can evaluate the bathyscaph and its research potentialities. A moment ago Dr. Dietz mentioned that the modification should be left for this afternoon's session. I don't feel this is true. I feel perhaps modifications for the scientific use of this should be perhaps discussed this morning in this discussion. I think this afternoon's program should be left strictly to the possible military potentialities of the bathyscaph. I think the program is fairly well set up along these lines.

I would like to briefly state what ONR's position is at this time. In fact, Jacques Piccard was over to Washington about a week ago and we discussed possible future operations with the bathyscaph, and it is ONR's

feeling at this time that what we should do as a minimum is continue, perhaps, over in the Mediterranean, the same sort of program which we carried out this past year. However, we feel that we would be on much better ground if we could bring the bathyscaph over to the United States and have it more in our own operational control. If it were brought to the Atlantic, for example, it would probably be given to one of the laboratories -- one of the Navy laboratories or one of the contract-operated laboratories -- and, in view of the deep water situation, would possibly be dived out of either Bermuda or San Juan. In the Pacific I think it would be very probable, if it were brought to the Pacific, it would be operated out of NEL, out of San Diego, because of the close proximity of Scripps.

There is also another possibility, that we might want to take this out and investigate the trenches with it. I think it is with a little bit of embarrassment we find they had to take advantage of the design of the Swiss Navy's bathyscaph and the Italian Navy's help, or else we would not have had the opportunity to use the bathyscaph.

DR. VINE: There are a few things I would like to say, to try to get the discussion going a little more. There are only a few people who went down in the bathyscaph.

From listening to their programs and what they did, you can see that several different kinds of things were done. I know that the people that went down had much bigger plans they would have liked to have attempted if time had been available. Probably half the people here would like to see other kinds of work done, that they are interested in, so it seems to me we should regard what we have heard this morning as merely being representative of the kind of things that can be done.

I have a few remarks on some of the things that were said. One was as to whether the physical oceanographer would have very much to learn from this with regard to temperatures and currents. I have a feeling that there is a great deal that can be done in observing the biological creatures in the ocean, because they are probably much more sensitive to the current and salinity changes and the temperatures than we are. Such things as sharp stratification, which the French had reported, may be occasioned by very small changes in temperature or salinity or current which they respond to, and have really quite a large effect on the way the ocean is made up, although to our instruments it doesn't look like a very important phenomena.

The second is: there were some remarks made as to the interference with sonar of the creatures in the ocean.

I think that when we use the word "interference" this is predominantly a negative term, and that what we should try to think of more is the positive term. The ocean is completely, to the scientist, a fact as it exists. To the military person it should be regarded as being completely neutral. For example, the oceans were an interference to land armies and the deep seas were an interference to the surface Navy, but it seems to me in the so-called interferences we should regard these as new regions and new areas of interest which we should explore.

MR. BASCOM: I'd like to speak for a moment about where I think this whole program might eventually be headed. It seems to me it is not too soon to think perhaps five or more years ahead in terms of what the ultimate vehicle might be for deep sea research and how we might go about getting this. It seems to me there are other ways of gaining support except by asking the Navy directly for funds, and one way would be to go to the United States public for money. Right now the climate is favorable. This is what the Piccards did, and look.

In order to interest the public you need visual items so they can grasp what you have in mind and visualize this and see what it is you want to do and hear your story, and I suggest that what the bathyscaph needs, in terms of equipment that you might put on the present one

in order to get results, are some things which are a bit gimmicky, in a sense, but in the end will do you as much good as the present scientific measurements you are making. One thing I have thought you needed was a set of prosthetic hands like those used in "hot" labs, so you can actually turn the bottom and poke at animals and hold nets and perhaps test things with it. I also think you need a much better photographic system. The present pictures that came back, taken from the inside, are lousy. What you need is a continuously operatable 35-mm camera with the best film you can get in it, with a lens-splitter as a lookout at the ocean all the time. Of what was taken this time Jacques Piccard brought what he thought was the cream of them, and he had ten pictures not of very good quality. If you want support, you have got to have good movies of what is going on at the bottom.

You want a bathyscaph that can be used as a corer, to take cores of the bottom at some place in the ocean where you are particularly interested, perhaps a large area of sea mounds, or some other particularly interesting facet of the bottom. This, of course, is quite a different vehicle from what you have now, but it's not too soon to plan for it, and, finally, I think you should have prepared a series of artists' conceptions, in the corniest sense of the words, of what this ultimate vehicle might

look like, and these should bear no relationship to your hopes of your chance of getting them today, but only as a means of capturing the public support.

MR. LILL: What are the two greatest limitations, or what are the greatest limitations in the present TRIESTE for scientific research? Can we hear some discussion on that?

DR. VINE: It would seem to me it has been pretty clear from the beginning, and it was brought up this morning, that mobility is the principal problem. This was compounded this summer because the first part of the mobility was getting to Italy, and the second part of the mobility was having to remain within a Sea State 2 when operating.

MR. LILL: That is the weather limitation.

DR. VINE: Yes, and then, when one was submerged, with the fact that the battery capacity was so low, one did not have much horizontal mobility.

MR. LILL: I would like to hear some comments from some of the engineers here on how you think it is possible to overcome the horizontal immobility. It seems to me we'd have to get a great deal more horizontal mobility in this, and I wonder if it is possible to do this on the present bathyscaph without too much expense and without too much design difficulty.

DR. BROWN: Was your comment directed to this?

MR. KIELHORN: The present bathyscaph has two air-buoyancy chambers located all the way forward and all the way aft, which are flooded with sea water to bring it down to neutral buoyancy. When these are pumped out and she is on the surface she floats, with a ... of about one foot and a half. Being a very crude shape, and being towed, it means everything you touch is coming aboard. To compound this difficulty, the conning tower is wide open, and the hatch is open, which means when being towed the seas which come aboard go directly in the conning tower, and it requires the hatch be closed at all times when being towed.

I believe you can get around some of its horizontal immobility, if you will, by merely closing this door here (indicating on blackboard), permitting perhaps access from aft instead of forward by at least a splash-proof door, and by giving a little more of a boat shape forward.

There is one danger in making this look like a boat. That is when it is submerged and rising. If you have a flat deck you are going to set up an oscillation so you will roll on the way up. Piccard has thought of releasing a parachute drogue to reduce the amplitude of oscillation. I am sure with the same hull you can get at least one sea state better out of it, and possibly two.

DR. LEWIS: Another difficulty in mobility occurs when you tow the bathyscaph. It also oscillates on the end of its tow-line. This shouldn't be too difficult to correct, either, I don't believe. The French have built keels on their's, which I think have solved the problem.

MR. KIELHORN: If we are thinking of this particular design. But the very addition of a stabilizing fin aft has helped immeasurably, and if there is any rudder control for use on the surface, attached to the stabilizer, I think it will tow fairly well.

____: It needs to be very much better than it is now. Apparently it could withstand towing in a high sea state, and at about a Sea State 5 the difficulty there was that the ballast tanks pounded too much. They are free so they can fall out easily, but some kind of emergency clamp on the ballast tanks to keep them in place in high seas should be available, and make them removable.

MR. KIELHORN: Those ballast tanks are about five tons apiece and they do slap, but I don't know if that is putting a tremendous strain on the hull. It makes a lot of noise because it's a piece of tin, but I don't know if it really damages.

MR. LILL: These are fine suggestions, but can we separate the power supply that runs this little engine

(indicating) from the power supply that runs the total bathyscaph, so that, for example, when you send the thing down to the bottom, and you have a ship on the top -- it could sink to the bottom and at the same time drive the bathyscaph along and see what the bottom really looks like that is giving certain effects on the top?

DR. VINE: It seems to me that the thing that can add the most to submergeability is to go to external lead batteries. This will provide more space inside and permit more capacity outside, and then one can either change the batteries out at sea or can give them a quick recharge from the mother ship.

MR. KIELHORN: Piccard had this very thing in mind in my last conversation with him. He said he would never put them inside again. His batteries are all open, so you can brush against the contacts. He definitely wants to keep his batteries out of that gondola.

MR. DIETZ: Piccard realizes this battery is just a make-shift. The French one has ten times more electric power. It has them external, which could be jettisoned, and the new super-bathyscaph, I think, will have 100 times more electric power. Also, Piccard's has a high tension system, and the French, I think, has a 28-volt system, which I think is much easier to handle, and it ties in with all their aircraft so they can use aircraft accessories

for their instruments. Piccard realizes, and everyone else does, that this is an obsolescent craft. Even when it was originally built they built this thing in terms of what money they could scrounge, and very small amounts at that, and this is no credit to modern technology. It never was, and certainly isn't today.

Several things can be done to improve the bathyscaph. It can be used, but we are very much better off to want a vehicle which pays for itself, research-wise. I don't think this one does. You can do several things. One thing which is widely known, of course, is to use different flotation substances, like lithium, which has a density of .54 compared to .70 and is rather compressible in water. Lithium is a solution, but recently the idea has been suggested to me in England -- and I know it is widely known here -- to use some lighter cabin, and the answer here seems to be to use a fiberglass cabin impregnated with a resin, and I have talked to people in England and some AeroJet people on the West Coast and they think this is feasible. This has a density of only 1.3 instead of 7.5, and it seems possible you could build essentially a buoyant sphere which could go to 2½ miles with our safety features. If it wasn't buoyant you'd only need a small amount of gasolene to work something like that.

Regarding the public sponsorship, the public is

already behind this thing. There is no difficulty in getting the public to support this. It's only necessary for the Navy and other people to realize the public is behind it and is behind them. As Allen Vine once told me, it is evident that the future development of submarines must be to go deeper and quieter, and there is ample military reason as well as ample scientific reason for doing this.

One more point is that the ocean bottom is only $2\frac{1}{2}$ miles away, and people are getting a lot of money now to go to the moon, which is 240,000 miles away. The ocean is only $2\frac{1}{2}$ miles away, and it's $3\frac{1}{2}$ times the living space of all the people combined.

DR. GALLER: It seems to me we are skirting one of the fundamental issues here. I would like to suggest that rather than go immediately into the gadgeteering end of how can we improve this particular vehicle we first reach some agreement, or at least put forth what we feel the research potential of this vehicle is. It seems, to be more specific, I would suppose, that really what we are talking about here are the developmental characteristics, but in order to reach the point of developmental characteristics let's set up some "operational requirements."

What are the research possibilities of the bathyscaph?

Now, as a biologist it seems to me there are at least two major general types of research capabilities of this vehicle: (1) taking advantage of this friendly depth that Dr. Dietz mentioned, to set up a "submerged anchor station" that would permit maintaining observations in situ, which is something that we cannot do today. Practically all of the data that we have received from the great depths is based upon pulling up samples and then analyzing them, either on board ship or maintaining them in vitro back in the laboratory, and this is not the same as studying an in situ situation.

The other aspect is the fact that, for the first time, it becomes possible to carry out three-dimensional integrated environmental measurements over a fairly long period of time, albeit within a limited space and distance, but to be able to carry out a considerable number of measurements simultaneously and correlate them with visual observations at the great depths is really -- this is what I would consider to be a technological break-through, from the standpoint of the bathyscaph, as far as bathypelagic biological research is concerned. Specifically, you mentioned -- or I think it was Allen Vine that mentioned it -- the possible relationship between organisms and minute changes in temperature. We have a great deal of information to suggest that there is a very definite

relationship. Herring, for example, may be caused to change major migratory patterns by changes in 1 degree Centigrade or less, and there is every indication that copepods may be influenced by changes of less than a half a degree Centigrade, so here we have a submerged laboratory with which, if we put on the proper gadgetry and are able to extend the life of the laboratory under water, we are able to do marvelous things we couldn't possibly do from aboard a ship on the surface.

DR. WENK: I was particularly interested in some of the earlier comments, particularly Dr. Dietz's, with regard to buoyant spheres. In another connection we have had occasion to study this matter in some detail, and I think you will be interested in the slides that will be shown this afternoon, to find that you can develop a vehicle that will operate satisfactorily at the same depths as the TRIESTE, that has positive buoyancy. It will not be necessary to go to some of the fragile and delicate buoyancy chambers that this present vehicle has.

I mention this now only because I think it is well worthwhile to recognize that the degree to which one pursues this interest in underwater life is likely to be conditioned by practical problems of sea state, and since this has come into so much discussion this morning I think

it would be worthwhile to consider re-evaluating the type of vehicle necessary to accomplish this mission. It is possible, of course, that one should proceed with an available vehicle, but, considering its many identifiable limitations, it might be worth taking another look at it, a completely different approach, and one that would make use of much more of the technology of structures and concepts that the Navy has developed, and recognize that you can get a submersible vehicle that will go to enormous depths satisfactorily.

MR. MAXWELL: Certainly I feel that we should proceed along both short and long term programs in this, and I think perhaps maybe the short-term program might involve further use of our existing capability, but I am quite convinced that in this we should not lose sight of a longer term objective of getting better vehicles, if this is a good technique.

DR. LEWIS: I would like to mention, before we get too far from the existing bathyscaph, that its utility could be greatly improved, or perhaps I should say its efficiency. The problem last summer was not only sea states. The biggest problem was actually the time it took to charge the batteries. This could easily be overcome, even with the present design, simply by having a number of sets of batteries on hand. If we had more than

one pilot to operate the bathyscaph and a number of sets of batteries, we could certainly get four or five times as many dives as we did this last summer.

MR. MAXWELL: Yes. I think it should be pointed out that we were working under about as adverse conditions as we could expect. There was a language barrier. We had to deal with the Italian Navy, and they at times cancelled the whole operation an hour before they were scheduled, and there were many living difficulties over there that hampered the program. I think its efficiency could have been increased by a factor of 5 by operating at American standards.

MR. LOMASK: In regard to the suggestion on the bathyscaph as being ideal for making biological observations in situ, the same thing occurs for many basic acoustic experiments as well. For example, there are acoustic velocity or acoustic attenuation studies which could be made in situ using the bathyscaph, and I guess up to now it's been done in laboratory measurements.

DR. VINE: I think a very important thing here with regard to the acoustics phenomena, or acoustic experiments, is that, if you have space to take along almost any one acoustician and his gear, he can try his experiment without a long drawn out period of development. One can certainly do a great deal of work on the acoustic work

with the ends of electric wires. This has been done and much more of it will be done.

Take this last summer's work, for example. The frequency range they covered was rather large, and they did this rather large range of frequency studies with relatively simple equipment. I think this is one of the ear-marks of having a laboratory that goes to depths rather than aboard a ship on the surface.

DR. FROSCH: There were several things, from an acoustic point of view. One already mentioned is dispensing with cables. This means that right away you can go to making measurements of relatively high frequency at deep depths without getting into a whole class of telemetering problems, which have cross-talk problems which have never been resolved for high frequencies. You also now have the possibility of a horizontal beam of known orientation and variable orientation. It also offers the possibility of fairly continuous measurement with depth, whereas with previous techniques, particularly for ambient noise, one could only take spot measurements with depth, because of the difficulty of moving the hydrophones up and down, because of their connections with the surface.

DR. RECHNITZER: In addition to this, if they only considered a second bathyscaph, then you have both the

sound source and receiver at the same level, and this means you can make your measurements at precise depths throughout the entire water column. This could be extended on to greater lengths if we had two bathyscaphs. Of course, from a military aspect I think this is quite essential that we have two of them.

MR. LILL: Mr. Chairman, there are people in this room that were brought here on purpose because they have the conviction that all these things can be done with unmanned vehicles, and they have many good points in their favor. Their points, I think, should really be made a matter of record in this discussion. I wonder if it's possible to get some of them to make some remarks?

DR. BROWN: Does anyone want to make any remarks along these lines?

DR. SPILHAUS: I think the complete justification of the bathyscaph is simply the irresistible urge of people which exists to go to remote places where they haven't been. Unfortunately, this is not a compelling enough argument for our people who have the money to develop these things. This is unfortunate because uses develop after you have been somewhere, so that the whole question of detail -- I have been extremely interested in listening to these secondary arguments of why you have to have people go to places, although I don't need them

at all to convince me. These are extremely interesting, but there are secondary things to that. If this primary argument isn't compelling to us, other people are going to beat us to it, so I feel this whole discussion of manned versus unmanned is completely secondary. It is my opinion that people are going to go wherever you send instruments.

DR. BROWN: Any other comments?

MR. COUPER: I'd like to point out that test vehicles of two bathyscaphs for things like plotting a beam pattern, which you can't do in the ocean now, because it takes a little more weight and handling capacity, would be very handy.

DR. RAFF: It seemed to me that the Number One problem was getting in and out of the bathyscaph in rough seas without having water go down the hatch. It seemed to me this is a very minor thing and would permit you to operate over the deck remotely -- no?

DR. RECHNITZER: Well, there are also other deck operations that have to be handled by the deck crew, and they are subject to being washed overboard in rough seas, so the opening of certain valves has to be done manually.

DR. VINE: For the purpose of clarification, I would like to state that while it was said water would go into the conning tower, this bathyscaph has normally

a wet conning tower and not a dry one.

MR. KIELHORN: In the same regard, talking about those valves opening fore and aft on those two compartments, it's a delicate operation. A man has to be on deck, and he is at the extreme end of the ship and, as such, gets overboard. These are very crude valves, screwed down by hand. There's not a reason in the world why a lever type valve can't be put on so that the conning tower is self-bailing, so that it will float.

DR. RECHNITZER: Also, you have to leave the mother ship to reach the bathyscaph, and this is hazardous.

MR. MAXWELL: I think there are more problems in rough weather than meet the eye here. I think the possibility of losing the bathyscaph during rough weather is part of the game. Certainly the Italian Navy did not want to go out in the rough weather because of handling operations, putting the lines aboard it, the compressed air tanks, and so on. It's a fair weather operation.

DR. VINE: It's a little bit like servicing seaplanes in rough weather.

It seems to me you can redesign to operate to perhaps one or two more sea states, and if you take it into an area which is predominantly good weather you can work half of the time instead of a quarter of the time.

DR. FROSCH: I'd like to jump back to Dr. Spilhaus's

remark and try to bridge it over to the practical point of view. One thing you are doing when you go down in this thing is that you are replacing rather inadequately controlled servo-mechanisms, which are the best we have got, namely with the scientist himself, who is sitting right on the spot and not at the wrong end of a not very adequate cable. In either direction he can decide what to do and know what is going on, rather than trying to deduce what might be happening to give him the peculiar result. This is the real thing. When you design something to go on the end of the cable and something odd happens, all you know is that something odd has happened, and you have a very small chance of discovering anything other than what you have built into the end of it, into the equipment at the end of the cable, whereas, if you are sitting there yourself you can correlate a lot more data than what you could possibly build into the end of the cable. You can get so much more information for a reasonable expense, rather than what you can get for any reasonable expense of cable.

DR. BROWN: In arguing this point a friend of mine made a very nice statement. He asked the question: what would the voyage of the BEAGLE have been like if Charles Darwin had been replaced by a motion picture camera?

DR. FROSCH: I don't think anyone here would care to

do any neomorphology from a balloon he could hang on the end of a cable.

DR. VINE: I think the present bathyscaph can serve to open up new problems. After we once learn enough about a problem, to where we can visualize it very well, then we can probably build an instrument that would do it better, but while we are still looking for different kinds and points of view about this ocean we work in it seems to me we need breadth rather than depth.

DR. REVELLE: The ability of training scientists to go down to depths around 200 feet to look at the sea floor has in fact given us a whole new level of understanding and raised a whole new host of problems, and instead of being in the top 200 feet you are opening up the whole vast depth of the ocean.

I would like to speak for a moment about the geological, what we can see would be the use of this to the geologists. The essence of geology is the use of the human eye. It is essentially an observational science. You have with this contraption the ability to look, and for a long time.

Just let me cite four different kinds of problems which would illustrate the advantages of being able to use the human eye. In both the Pacific and Atlantic there are many ground ancient islands, islands known now, at

depths of 3000 to 6000 or more feet. The question is: just what were these things like when they were at the sea surface. Were they gradually cut off from the sides? We are almost certain they were islands that were cleaned off by the wave erosions, but then what was the subsequent history? Did they remain at the surface or near the surface so that something grew on them? Were there beaches on them? We know they were islands, because we have collected shallow water reef fossils of animals that could only have lived at the surface, but we don't have any clear idea of their history when they were islands. This gives a great deal of inference that this early history could be obtained if you could go down and look at them.

Secondly, at Eniwetok there are now on the bottom fairly course sediments with ripple marks which look like wave ripple marks, which we think were caused by the periods of the order of 20 seconds at depths of somewhere around 1500 to 2000 meters. This is completely impossible. There aren't supposed to be any waves of a period of 20 seconds big enough to move the sands around on the bottom and cause ripple marks. Just below the sand layer there are very fine-grained clays, clays that fossils indicate are Pliocene or older, or Neocene. What is it about this area that in the past allowed very fine

sediments to accumulate and now allows only sparse amounts of coarse sediments, which apparently have been shifted recently by rather short period waves? One question, for example, is whether these waves now exist or whether they existed during the Ice Age, when perhaps there was a great quantity of material being washed out of the lagoon of the coral atolls and perhaps causing great turbidity currents. We'd like to see whether these are now being formed or whether they are fossil.

The third is the problem of sticking spears into the bottom, or instruments for measuring the heat flows of the sea floor. How do these contraptions work? There is a problem of measuring the heat flow. It's quite a difficult one, because the cable must be paid out and the thing must be paid out. What happens to the cable during this time? Does this get pulled sideways? What happens to it? So if you look at the instruments you lower from a ship and see how they work, it's a third very important geological thing. This has been already effectively done down to 200 feet with the aqualung.

The fourth is to actually make geological experiments. One Dr. Dietz already mentioned is a beautiful example, to see if it is possible to cause and follow a turbidity current. They so far are almost entirely theoretical. We think they must exist, but nobody has actually seen

one, although we have seen things that might possibly be turbidity currents. Also, there is the question of their velocity and relationship of velocity to density and the whole transient state, and how fast they change, and things that I think we just have to observe, so in four different ways, (a) studying geological structures, (b) studying geological processes, (c) studying the working of geological instruments, and (d) making geological experiments, this thing is potentially an extremely powerful geological tool. It depends essentially on what you call a good servo-mechanism, and I would prefer to call it the use of a human eye to look at it and see what is happening. The geology data of the earth would be considerably more primitive than they are if we had to make all our observations by lowering long steel fingers.

DR. BROWN: I believe that our time is running out, and we are going to have to bring this morning's session to a close. I am supposed to summarize what has been said and, as you can see, that is a physical impossibility.

I believe as a result of the morning's session we have pretty general agreement among the scientists that vehicles of the bathyscaph type can give us the possibility of making new kinds of measurements that could not be made before, and new kinds of observations that

could not be made before. In addition, I believe there is general agreement that the present bathyscaph can be modified to make it more generally useful and that the present bathyscaph design should be more or less looked upon as a counterpart of the Wright brothers' plane, that there are many improvements that can be made, and, if a program of some sort is established, that they probably will be made.

I believe that we are agreed that a new kind of deep sea vehicle can be built which will extend the range and the depth and the scientific flexibility.

The major question that we have not answered, and which perhaps we can discuss this afternoon after we discuss the military aspects, is the extent to which the United States should become involved in a program of this sort. Do they find profits that are rather obvious justification for the expenditure of rather substantial amounts of funds which will be necessary?

With that I think we will close, and I believe we are supposed to get started, according to the program, at 1:45. We are starting a little late, so can we make it 2:00 o'clock? All right. 2:00 o'clock.

(Thereupon, at 12:25 o'clock, P. M., the meeting was adjourned for lunch.)